with a four-foot aperture; and therefore it is no wonder if I find nights of the requisite degree of tranquillity somewhat more rare. Yet I find my own physical strength insufficient to allow me to use up half the quantity of available sky, and my next want will probably be some efficient and energetic assistance in the duty of observing.

To General Sabine, I remain, &c.,
President of the Royal Society, &c. WM. LASSELL.

II. "On the Theory of the Motion of Glaciers." By WILLIAM HOPKINS, Esq., F.R.S. Received April 14, 1862.

(Abstract.)

Almost all the numerous discussions which have taken place during the last twenty years respecting our theories of glacial motion have had for their object the assertion of some particular view, rather than the establishment of a complete and sufficient theory founded on well-defined hypotheses and unequivocal definitions, together with a careful comparison of the results of accurate theoretical investigation with those of direct observation. Each of these views has been regarded, improperly, in the author's opinion, as a *Theory of Glacial Motion*. The Expansion Theory ignored the Sliding Theory, though they were capable of being combined; the latter theory was equally ignored by the Viscous Theory, in which, moreover, instead of the definitions of terms being clear and determinate, no definition of *viscosity* was ever given, though that term designated the fundamental property on which the views advocated by this theory depended. Again, the Regelation Theory is not properly a theory of the motion of glaciers, but a beautiful demonstration of a property of ice, entirely new to us, on which certain peculiarities of the motions of glaciers depend.

When we shall have obtained a *Theory of the Motion of Glaciers* which shall command the general assent of philosophers, no qualifying epithet will be required for the word *theory*; it would indeed be inappropriate, as seeming to indicate the continued recognition of some rival theory. If, for instance, it should be hereafter admitted that the sliding of a glacier over its bed and the property of regelation in ice are equally necessary, and, when combined, perfectly

sufficient to account for the phenomena of glacial motion, there would be a manifest impropriety, not to say injustice, in selecting either of the terms *sliding* or *regelation* by which to designate this combined theory. The author makes these remarks because he believes that the preservation of the partial epithets above mentioned has a tendency to prevent our regarding the whole subject in that more general and collective aspect under which it is one of the principal objects of this paper to present it.

This object must necessarily give to the present paper something of the character of a *résumé* of what has hitherto been done, whether it be our purpose to adopt or reject the conclusions of others. There are periods in the history of almost every science when its sound and healthy progress may almost as much demand the refutation of that which is erroneous as the establishment of that which is true. It is not intended, however, to enter into any review of the past labours of glacialists with respect to exploded theories, but only to notice those more recent researches and speculations which appear either to demand refutation as erroneous, or to be admitted into any well-founded theory as correct.

With a view, in the first place, to remove the ambiguities which have beset this subject from the want of explicit definitions, the author enters into the following discussion and explanation of terms employed to express properties of ice on which our theories of glacial motion must essentially depend.

1. The external forms of all bodies in nature may be changed in a greater or less degree, and without producing discontinuity in their mass or destruction of their internal structure, by the action of any external forces, the original or undisturbed form from which the change of form is to be estimated being that which the body would assume if acted on by no external forces whatever. This change of form necessarily implies a change in the relative positions of the component particles of the mass, or a certain greater or smaller amount of *molecular mobility*, or power in the particles of moving *inter se*. We may speak either of the general change of the form of the whole body, or of that which takes place in each of its small elementary portions; it is, in fact, in this latter sense that we are obliged to regard it in any accurate investigations, because the change of form for different elements will usually be different. Change of

form in an element may or may not be accompanied by a change of its volume. In the first case it leads to *cubical* extension or compression; in the latter, merely to extension or compression of the surface and not the volume of the element. It may be called *superficial* extension or compression. These changes of volume and form in any element must be produced by the forces acting on it. Thus we may conceive linear extension alone produced at any interior point of the mass by two equal and opposite tensions acting on two elementary component particles there in the direction of the line joining their centres of gravity, while compression alone would result if those tensions were changed into pressures. In such cases extension or compression would be the result of forces which may be called *direct* or *normal* forces. In the case above mentioned, in which the volume and density of every element of the mass remain unaltered, there can be no such direct normal action as that just mentioned. It must be perpendicular to the normal action, and therefore a *transversal* or *tangential* action. There would be no tendency to make the contiguous particles approach to or recede from each other, but to cause the one to *slide* tangentially past the other.

If the body have a structure like that of any hard, vitreous or crystalline mass, pressure at any point will tend to break or crush the body, and thus to destroy the continuity of its structure. This tendency will be opposed by the *resisting power* of the substance. The tendency of the direct or normal tension is to separate the contiguous particles, and thus produce a finite fissure, or a discontinuity in the mass. It is resisted by the *normal cohesive power*; and in like manner the transverse or tangential action is resisted by the *tangential cohesion*, or that which prevents the component particles from sliding past each other. Again, when the component particles at any point of a body are relatively displaced, they have always a certain tendency to regain their originally undisturbed position, and the force thus excited, considered with reference to the force of displacement at that point, affords a measure of what is called the *elasticity* of the body. Since the force of restitution may vary from zero to the corresponding force of displacement, the elasticity, when measured by their ratio, may vary from zero to unity.

2. We may now define such terms as *solid, plastic, viscous,* and the like, with all the accuracy which their definitions admit of. We

may call a body emphatically a *solid* body when it possesses the following properties :—(1) small extensibility and compressibility, (2) great power of resistance and great cohesive power, both normal and tangential, and (3) great elasticity. It will thus require a comparatively great force to produce any sensible relative displacement among the constituent molecules of the body; if we conceive the force required to become infinitely great, we arrive at absolute *rigidity* as the limit of solidity. Again, we shall best, perhaps, define *plasticity* or *viscosity*, if we suppose the forces of displacement to be such as to produce only a small transverse or tangential displacement of the constituent particles, *i. e.* a superficial, not a cubical, extension or compression. Then if the force of restitution bear only an inappreciable ratio to the corresponding force of displacement, *i. e.* if the *tangential elasticity* be not of sensible magnitude, the mass may be emphatically said to be *plastic*. This is the essential condition of what may with strict propriety be termed *plasticity*; it might also be added that, as bodies are constituted in nature, the force required to produce the original displacement in plastic bodies will be small as compared with that required in solid bodies. Viscosity and semi-fluidity are terms which only express similar properties of bodies, but usually indicating that still smaller forces only are required to produce a given displacement in such bodies than in plastic ones. The limiting case is that of perfect fluidity, in which both the forces of original displacement and those of restitution are indefinitely small. In these latter cases the tangential cohesion is necessarily small, and such also (as bodies are usually constituted) will be the normal cohesion. At the same time the power of resisting compression of volume may be very great, as in fact it is in nearly all masses not technically designated as *elastic masses*. In other words, the *normal* elasticity, with reference to pressure, may be of any magnitude, while the *tangential* elasticity equals zero.

It will be observed that a body is here spoken of as held in a state of constraint by internal forces, but without any kind of dislocation which should destroy its continuity or injure its structure. If, however, the external forces should be sufficiently increased, the structure of a vitreous or crystalline mass, or that of any mass possessing hardness and brittleness, will be destroyed by a pressure greater than its power of resistance can withstand; or the continuity of its mass will

be destroyed by any normal tension greater than the normal cohesion; or, again, by any tangential tension greater than the tangential cohesion. The normal tension would thus produce an open fissure; and the tangential tension would cause one particle of the mass to slide past another, but without producing any open discontinuity. On the contrary, in a properly plastic or viscous mass there is no definite structure for excessive pressure to destroy; there is no question as to the formation of open fissures; and the characteristic absence of *tangential* elasticity allows of any amount of change in the relative positions of the constituent particles of the mass without breach of its continuity.

It would of course be impossible to draw an exact and determinate line of demarcation between solidity and plasticity, but it is not therefore the less certain that there are bodies which do unequivocally possess the property of solidity, and others which do as unequivocally possess the property of plasticity, according to the definitions here given of these terms. Solidity and plasticity with respect to numerous cases in nature thus become determinate properties of those aggregates of material particles which we call bodies. Ice, a vitreous or crystalline and brittle mass, which will neither bear any but the smallest extension without breaking, nor more than the smallest compression without being crushed, must be solid, and cannot be plastic, if we are to use those terms as significant of determinate properties of bodies.

3. The advocates of the viscous theory would not probably admit the necessity of the above rigorous definition of the term viscous in its application to glacier ice. But the defect of that theory has always been in the entire want of any accurate definition of that term. When such definition was demanded, it was said that glacier ice must be viscous, because a glacier adapted itself to the inequalities of its valley as a viscous mass would do. This was equivalent to saying that the mass was viscous because it moved in a particular manner, instead of asserting that the mass moved in that particular manner because it was viscous. Now this kind of inversion of the direct enunciation of the proposition is only admissible when there is no other physical cause than the one assigned, to which it is conceivable that the observed phenomena should be ascribed. Thus we may assert with perfect conviction, that gravity exists as a property of

matter and acts according to a certain law, because the bodies of the solar system move as if such were the case; but the conclusiveness of this inductive proof of the proposition—that "gravity is a property of matter"—rests entirely on our conviction that matter has no other property by which we could equally account for the phenomena of the celestial motions. And so with regard to glaciers. If viscosity were the only conceivable property of ice by which we could possibly account for the observed motion of glaciers, then would the observed phenomena of that motion perfectly convince us of the existence of the property in question. But here the two cases entirely differ, inasmuch as there was no general conviction, nor even a decided probability at the time I allude to, that no physical property of ice could exist besides viscosity which might account for the observed phenomena of a glacier's motion; and at the present time it is proved that there *is* another property of ice by which those phenomena are perfectly accounted for, and the inductive proof becomes altogether valueless. Moreover, in the case of universal gravitation, the inductive proof is the only possible one, whereas in glacier motion we are concerned with a property which, in whatever sense the definition of it may be regarded, must be as capable of being rendered patent by experiment in ice, if it exist, as in any other substance.

The answer, then, that was given to the question—what is viscosity?—comprised no definition at all of that term. The viscous theory ignored the possibility of the molecular mobility of a glacial mass united with the preservation of its continuity, being attributable to any other property than that which was designated as *viscosity*, but without giving any exact definition of the term. If it was meant to define by it the property which is here defined by the same terms, the theory had a legitimate claim to be considered a *physical* theory, because it assigned a determinate physical property as the cause of certain observed phenomena. In this sense, however, the author conceives that it would now be admitted to be entirely disproved by Professor Tyndall's experiments, in which the ice exhibits so clearly the property of solidity, and the absence of all indication of plasticity. It may be presumed that the hypothesis of viscosity could only have been adopted in the first instance from the apparent absence of any other property of ice which might account equally well for the molecular mobility of the glacial mass.

4. But if the determinate property of viscosity, as here defined, be not recognized in ice, what, it will be asked, is really the idea which has been attached to the term plastic or viscous? The question, as already observed, is difficult to answer. Perhaps the best way of doing so is to refer to the *Prefatory Note* to Principal Forbes's 'Occasional Papers' (p. xvi). He there intimates that the expressions "bruising and re-attachment," and "incipient fissures re-united by time and cohesion," used by him in 1846, are to be regarded as having the same meaning as the expression "fracture and regelation," first introduced into the subject in 1857. Now there is no ambiguity whatever in this latter expression. "Fracture" means the breaking and splitting of the ice regarded as a brittle and crystalline *solid*, and could never be intended to have the slightest reference to viscosity. In fact the expression is altogether inapplicable to any body which can be called viscous without a violation of scientific language. Still this, it may be said, may be only a want of strict accuracy of expression, rather than of accuracy of conception. But if a notion of cracking and breaking, so foreign to any idea of plasticity, should be admitted, it could not be said that a glacier moved as it is observed to move, because it was plastic, but merely that it moved *as if* it were plastic. The true inference from the motion would have been that glacier ice possessed not necessarily real plasticity, a definite property of bodies, but a *quasi-plasticity*, which expresses no determinate property at all, but may consist with many different properties. It merely expresses, in fact, the power of the component elements of the mass of changing to a certain extent their relative positions. But this is not the peculiar property of ice; it is common, indeed, to all bodies exposed to disruptive forces which, as in the case of ice, the cohesive power is unable to withstand. The mass of any other substance, as well as that of a glacier, will then be broken into fragments sufficiently small to allow it to follow the impulses of the external forces acting on it. To say, therefore, that a glacier moves *as if* it were plastic is not to assign to ice any property peculiar to itself, and therefore does not properly constitute a *physical* theory of glacier motion at all.

5. But if we pass over the difference between true plasticity and that which, as we have pointed out, is merely apparent, there still remained the great difficulty, which was only removed by the experiments of Mr. Faraday and Dr. Tyndall. Every one who believed ice

to be a solid body, believed as a matter of necessity that a glacier must, on account of the external conditions to which it is subjected, be excessively broken and dislocated in the course of its motion. The author was himself one of those who fell into the error of attributing too much influence to the larger and more visible disruptions of the mass; but the great difficulty was in the perfect subsequent reunion of portions which had thus been separated, whether by larger or smaller dislocations. And here it will necessarily be asked whether, in the expressions above quoted, "*re-attachment*" and the "*reunion* by time and cohesion" of separated portions when again brought into contact, really mean the same thing as *regelation*? This question the author thinks can be answered only by saying that, whatever might be the intended meaning of those expressions, they failed to convey to the minds of others the most remote idea of regelation, as a property of ice at a particular temperature. No better proof can be given of this than the general conviction which appeared to flash across the mind of every glacialist when he first heard of Professor Tyndall's experiment, that the recognition of the property of instantaneous regelation was a well-marked and important discovery, which had at once completely removed a great stumbling-block in glacial theory. In fact, the viscous theory assigns no physical cause for the reunion in question. All we could do, before the publication of those experiments, was to infer from the observed facts that ice did possess some property which facilitated the reunion of separate pieces in contact; but this was like the attempt to define viscosity by an appeal to the phenomena which that property was intended to explain. Regelation has, in fact, no connexion with viscosity, but stands in direct antagonism to it.

An *imperfect plasticity* in ice has sometimes been spoken of. The fact is, all solid bodies may be said to have an imperfect plasticity, if we chose to admit this vagueness in scientific language, since all are capable of greater or less extension or compression. As to the apparent plasticity inferred from the motion of glacial masses, and arising from the crevicing of the ice as already explained, it has no relation whatever to real plasticity. Such crevices are the necessary consequences of the external forces acting on the glacier, and are as essential to the theory of regelation as they are unconnected with any property of plasticity.

The author then briefly describes the experiment, by which it is shown that ice will slide down an inclined plane at an inclination to the horizon less than that of any known glacier, provided its lower surface be in that state of disintegration in which it will necessarily be when its temperature = zero (C.). The motion is then slow and uniform. That glaciers do slide over their beds, has been established as clearly as it can be by the comparatively few observations which have been made on the subject; and every existing glacial valley, and every valley which is believed to have been such at former geological periods, testify to the truth of that conclusion. The author also explains that both theory and observation agree in the result that the temperature of the lower surface of a glacier of any considerable depth in the latitude of the Alps must necessarily be = zero (C.). He regards this sliding motion as far too important a part of the whole motion of a glacier to be neglected in any complete theory of that motion.

The author then proceeds to investigate certain properties of the internal tensions and pressures at any point (P) in the interior of a mass held in a state of constraint by external forces. He shows that at every point (P) there are three determinate directions, at right angles to each other, in which the direct tension is such that in one of them it is a maximum, in another a minimum, and in the third neither a complete maximum nor a complete minimum; it is convenient to call this the *mean* axis. The tensions or pressures in these directions are called *principal tensions* or *pressures*; there are also two other directions through P characterized by a peculiar property. If we take two adjoining particles, P and P′, in the line of maximum tension, that tension will exert a greater effort than there will be in any other direction to separate those particles; or if the internal force be the maximum pressure, those points will be more compressed together than in any other direction. In the two directions (now to be defined) the forces on P and P′, acting perpendicularly to the line joining those particles, will exert a greater tendency than is exerted in any other direction, to separate them by making one *slide tangentially* past the other, and then to twist and contort any internal elementary portion of the mass. These two directions are perpendicular to each other, and bisect the angles between the directions of maximum tension and maximum pressure. This problem

is treated entirely mathematically; it is the *typical* problem of this part of the subject. The results are applied to a real glacier by the analogy which it bears to the typical one.

For the application of these analytical results, the author then considers the nature of the forces called into action by the two primary characteristics of the motion of a glacier—that its central move faster than its marginal portions, and the portions near the upper faster than those near the lower surface of the mass. He also takes account of the modifications to which these forces may be subjected by changes of form and inclination in the containing valley. He likewise explains the different modes in which the mass may be fractured when the forces become such as to overpower its powers of cohesion or resistance. If the cohesion give way to the maximum tension, an open fissure must be formed in a direction perpendicular to that tension. If the *resisting power* of the ice give way to the maximum pressure at any point, the mass will be *crushed* at that point, but its continuity will be immediately restored by regelation, the internal constraint will be momentarily removed, and the mass will move on. By a repetition of this process the glacier is enabled to move forward, preserving at once the continuity of its motion, of its mass, and of its structure.

The *veined structure* of glacial ice is then examined, and it is shown that, so far as Professor Tyndall's *pressure theory* of that structure involves the condition of the structural surfaces being perpendicular at each point to the maximum pressure there, it is perfectly accordant with the theoretical results of this paper. Whether the structure be marginal, longitudinal and central, or transversal, this is equally true, assuming always that the structure in each locality is the direct and immediate consequence of the forces acting there and tending to produce it. Probably, however, the veined structure in one locality may have originated in another from which it has been *transmitted* by the motion of the glacier. Supposing this to be so entirely, the author examines how this motion of transmission would modify the forms of the transmitted structure. Practically, and within the limits to which observation has yet extended, these modifications would produce forms sensibly coincident with those which would result, as in the previous case considered, from the immediate action of the forces, independently of transmission. The respective effec-

tiveness of these two causes, therefore, in producing the veined structure in any particular locality is not at present determined. Its determination would require more accurate and detailed observations than have yet been made on this subject.

The *differential theory* of the veined structure is then considered; but here the author dissents entirely from all Professor Forbes's mechanical reasoning, by which he professes to determine the positions of the surfaces of maximum differential motion, which, according to this theory, are coincident with the structural veins. Mr. Hopkins contends that the actual differential motion of two contiguous particles must necessarily take place in the common direction of their motions. He cannot understand the effectiveness of such motion in any other sense, in producing the phenomena in question. He has investigated for this case the forms of the veined surfaces, but finds them altogether different from the observed forms; and with respect to Prof. Forbes's investigation he cannot possibly admit it, as he at present understands it.

The author then examines the *intensity* of the *dislocating forces* acting on the glacier. He demonstrates Prof. Forbes's error in supposing that it is much augmented by an enormous hydrostatic pressure within the mass, tending to push it onward in the direction in which it may be most free to move. It is proved that, under the existing conditions of a glacier, the hydrostatic pressure from the water contained in the pores of the mass can but little exceed the atmospheric pressure on its surface. But Mr. H. shows that there must in many localities be a very large increase in the intensity of the internal tensions and pressures arising from the free sliding motion of the whole glacier. Where the motion of a particular part of the mass is retarded by local circumstances, there will probably be an enormous pressure upon it *à tergo*, from the mass behind; or there may, in other cases, be a great additional tension, arising from the freer motion of the mass in front. Hence the dislocating forces must often be greatly increased, the dislocation is ensured, and the operation of regelation brought into action; and the continued motion of the glaciers is preserved when it might otherwise be arrested.